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Abstract

There is growing rigorous evidence on the schooling impacts of unconditional cash transfers, but only few have systematically reviewed the literature. This paper fills the gap through applying a meta-regression analysis to 38 studies of 22 programmes in 18 countries. We find that unconditional cash transfers improve both student enrolment and attendance, and the result is robust to the exclusion of studies with a high risk of bias. We also find statistically significant heterogeneity in effect sizes across studies. The effect on enrolment is larger for setting where average monthly labour income in the economy is lower and for secondary school students. However, we do not find other moderators in this paper, namely transfer size, whether the programme is pilot, and poverty head headcount ratio, explain the variation in effect sizes. Our paper highlights the need of more evaluations on the schooling impacts of unconditional cash transfers and how tweaks in programme design could make a difference.

Keywords: unconditional cash transfer; school enrolment; school attendance; meta-analysis

JEL classification D04 · D10 · I20

1. Introduction

"By 2030, ensure that all girls and boys complete free, equitable and quality primary and secondary education leading to relevant and effective learning outcomes."

– Target 4.1, Sustainable Development Goals.

Most developing countries are behind the goal of universal primary education (let alone universal secondary education). Despite some progress, nearly one-fifth of the global population of children and youth were still out of school in 2018 (United Nations 2020) and out-of-school rates stood at 33.6 percent in low-income countries in 2020 (UNESCO Institute for Statistics 2021). Moreover, educational inequalities among population groups are large: for example, in low-income countries, the primary school completion rate for children from the poorest 20 percent of households was 54.8 percentage points lower than that of the richest 20 percent of households in 2014-2018; similar disparities were found in completion rates for high schools (United Nations 2020). More worryingly, the COVID-19 pandemic may have undermined the progress on access to education and widened existing educational inequalities (United Nations 2022).

There is strong empirical evidence suggesting that "demand-side" interventions can increase student participation in schools by increasing income of households (an income effect) and/or decreasing the opportunity cost of schooling (a substitution effect), particularly for financially constrained households (Yang 2008; Baird et al. 2014; Simões and Sabates 2014; Glewwe and Muralidharan 2015; Blimpo et al. 2019). Programmes that pay for school fees have been found to improve school enrolment and attendance in developing countries (see e.g., Deininger 2003; Borkum 2012; Blimpo et al. 2019; Brudevold-Newman 2021). However, participation could be less sensitive to fee eliminations or reductions in the presence of high labour market opportunity cost (Garlick 2013) and other costs for children's education (J-PAL 2017; Sakaue 2018).[1] Therefore, in addition to removing these costs, providing subsidies to households are also ways to encourage school participation.

Of all the ways, conditional cash transfer programmes (CCTs), which are now present in 61 countries (World Bank 2018), have been widely tested and found effective to increase participation in many developing countries (Glewwe and Kremer 2006, Glewwe and Muralidharan 2015; García and Saavedra 2017; J-PAL 2017; among many others). However, targeting and conditionality make CCTs expensive to administer (Benhassine et al. 2015; Özler 2020) and potentially exclude the neediest groups by discouraging households to even apply for it (Baird et al. 2011; Freeland 2007). Moreover, theoretically, conditions could distort optimal decision-making if households are fully rational (Hanlon et al. 2010). If education is a normal good and financial constraints are the only barrier, households may invest in education optimally when they become richer without the need of attached conditions to cash transfers. Another possible issue is households may misunderstand conditionality if CCTs are complex, making the benefits of conditionality obscure (Benhassine et al. 2015).

Would unconditional cash transfer programmes (UCTs) do an equally good – or perhaps an even better – job in increasing student participation? There are mixed results on conditionality. Baird et al. (2011) compare, through a randomised experiment in Malawi, the effects of unconditional transfers and transfers conditional on school attendance on adolescent girls' participation, human capital formation, marriage, and childbearing. They find CCTs have a larger gain in enrolment than that of UCTs and a modest yet significant effect on learning. However, UCTs reduce dropout rates, an effect that is 43 percent as large as CCTs'. Besides, without conditional on attendance, UCTs can reach out to girls who had dropped out and reduce

[1] Other costs include, for example, textbooks, uniforms, informal user fees and travel time.

their rates of teen pregnancy and early marriage. In Morocco, Benhassine et al. (2015) find that a labelled UCT for education purposes that features small transfers, targeted poor communities, and paid out to fathers has positive effects on participation. In the absence of conditionality, the transfer increases the demand for education through parents' perceived returns to education and pure income effects (Benhassine et al. 2015; García and Saavedra 2016). More children in a household will go to school if the income effect is large enough and stronger than the substitution effect (Ferreira et al. 2017; Churchill et al. 2021). Baird et al. (2014) highlight that, in a meta-analysis, both CCTs and UCTs improve participation compared to no cash transfer programmes; although the effect sizes are always larger for CCTs compared to UCTs, the difference is not statistically significant.

Despite relatively small, rigorous evidence on schooling impacts of UCTs is growing, but only few have systematically reviewed the literature (Baird et al. 2014; Bastagli et al. 2016). This paper fills the gap by conducting, to our knowledge, the first meta-analysis of the field to examine the overall effects of UCTs on enrolment and attendance. We meta-analyse 38 studies of 22 UCTs in 18 countries and find that UCTs improve both enrolment and attendance. However, the heterogeneity in effect sizes between studies is statistically significant. Our paper also focuses on the question of what factors are responsible for the heterogeneity. We answer the question by considering risk of bias of each paper and the moderating effect of whether the UCT is a pilot programme, transfer amount as a percentage of average monthly labour income in the economy, average monthly labour income in the economy (hereafter average labour income), poverty headcount ratio, gender of the UCT recipient, and children schooling level in the meta-regression. We find that the basic result is robust to the exclusion of studies with a high risk of bias and the effect of UCTs on enrolment is larger for setting where average labour income is lower and for secondary school children.

Our contribution is threefold. Firstly, we respond to calls for increasing the evidence base for UCT evaluations (Baird et al. 2014; Bastagli et al. 2019). We provide a synthesis of mixed empirical evidence on schooling impacts of UCTs, address heterogeneity in the UCT estimates, and examine publication bias. Meta-studies on UCTs are scant: the few studies such as Pega et al. (2017) and Siddiqi et al. (2018) focus on health and nutritional impacts of UCTs; Baird et al. (2014) and Bastagli et al. (2019) are the only two meta-analyses that cover schooling impacts but of five UCTs only. Our paper, by contrast, focuses solely on and more UCTs (22 UCTs across 18 countries). We add 24 new studies to Baird et al.'s (2014) analysis sample, and the overall effect sizes we get are relatively small. A possible reason is that, in Baird et al. (2014), the large positive effects observed could be a result of UCT recipients' misunderstanding of schooling requirements. Some examples include Ecuador's *Bono de Desarrollo Humano* and Kenya's CT-OVC that have a high number of recipients reported a misunderstanding of the programmes (Schady and Araujo 2008; Ward et al. 2010; Edmonds and Schady 2012).[2] More recent studies, Araujo et al. (2017) for example, find a much modest effect size in *Bono de Desarrollo Humano*. Our finding suggests that evaluations of children or schooling UCTs need to take into account perceptions of rules and conditions through more rigorous assessments. Evaluating more UCTs that do not directly involve children (such as BONOSOL pension programme in Brazil) can also help address the issue of misperceptions of rules and conditions.

Second, following Baird et al. (2014) and García and Saavedra (2016), we examine whether programme design features contribute to the heterogeneity of UCT estimates; we also add two country-specific factors – average labour income and poverty headcount ratio – to the

[2] For example, 25 percent of *Bono de Desarrollo Humano* recipients in Edmonds and Schady's (2012) sample and 50 percent of Kenya's CT-OVC recipients (see Ward et al. 2010) thought that the transfers were conditional on enrolment.

sources of the heterogeneity.[3] In line with the two studies, we find transfer size, whether the UCT is pilot, and the gender of transfer recipient matter little, suggesting that: (i) changing the intensity of the income effect by varying the transfer size may be irrelevant to UCT effectiveness, (ii) there may be negligible differences in the effect between national programmes that influence labour supply decisions through their impact on permanent income and pilots that do not, and (iii) men and women may have similar preferences about spending on their children's education.[4,5] This adds support to the need of more evaluations on how tweaks in programme designs could make a difference (Benhassine et al. 2015; Glewwe and Muralidharan 2015; J-PAL 2017).

Third, motivated by a recently developed theoretical framework for UCTs by Churchill et al. (2021), the two country-specific factors we use are important for household decision-making but have not been explored in previous meta-studies. Their UCT model predicts that average labour income in the economy and the proportion of people living in poverty in the economy affect the magnitude of the estimate of transfers. We find average labour income matters but only for the effect of UCTs on enrolment. This, again, highlights the need of considering programme design details in UCT evaluations.

The paper proceeds as follows: Section 2 synthesises theoretical literature of household investment in human capital and empirical evaluations of UCTs; Section 3 reports the process

[3] Baird et al. (2014) include studies on both CCTs and UCTs while García and Saavedra (2016) focus on CCTs only.

[4] Theoretically, cash transfer programmes should affect labour supply decision (hence children's schooling) through its effect on permanent income. Transitory cash transfers, in contrast, should have no effect on labour supply decisions at the time the transfer is received.

[5] In general, conditional transfers are made to the mother in a household with the assumption of unearned income in the hands of mother should lead to greater human capital investment in children (Baird et al. 2014; García and Saavedra, 2016). However, Benhassine et al. (2015), Akresh et al. (2016) and Bastagli et al. (2016) find no conclusive evidence of who gets the transfer matters.

and result of literature search; Section 4 describes the meta-regression model; Section 5 discusses the results; and Section 6 concludes.

2. Schooling Effects of Unconditional Cash Transfers

The economic model of schooling decisions is built on household decision-making models or, generally, on the theory of consumer choice. Demand for schooling is a choice between current and future consumption subject to households' budget constraint. Households receive income from nonemployment income – labour income of adult men in the household, child labour, and mother labour – and a fraction of future earnings of children as adults; the sum is equated to expenditure on consumption, including the investment in human capital (García and Saavedra 2016).[6] The demand depends on the expected benefits to schooling (e.g., children's future earnings) and the present discounted costs of schooling (direct costs – e.g., fees, books, transportations, etc. – and children's forgone earnings) (Becker's (1962; 1975) and Ben-Porath's (1967) household decision-making models). Put it differently, it involves a trade-off between children's earnings from labour and education (Mincer's (1974) human capital earnings function). When child labour is a substitute for adult labour, two equilibria exist in the labour market: (i) where children work and (ii) where adult income is high and children do not work (Basu and Van, 1998) – that is, more children in a household will go to school if adult income is high enough (Ferreira et al. 2017; Churchill et al. 2021); child labour arises in equilibrium because of household financial constraints (Baland and Robinson 2000).

Ferreira et al.'s (2017) model of schooling decisions predicts that cash transfers and conditions could work through: (i) a positive income effect where they increase enrolment of

[6] See García and Saavedra (2017) for two other constraints – a human capital production constraint and an adult earnings production function – that households face. We focus on budget constraints only to explain the 'income effect' of UCTs.

all children in the household; (ii) a positive substitution effect through reducing the opportunity cost of schooling for children. However, if not all children are eligible for the programmes, a third effect could emerge – a positive displacement effect in which eligible children displace their ineligible siblings from school. This implies that Basu and Van's (1998) two equilibria in the labour market depend on which effect (income versus displacement) dominates and that the income effect must be large enough for all children in the household to enrol in schools. Specifically, ignoring the conditionality, cash transfers by themselves could increase nonemployment income of adults, and a greater transfer should lead to greater increased student participation – that is, through a pure income effect (García and Saavedra 2016).

More recently, Churchill et al. (2021) introduce a model that examines the effects of UCTs financed by labour income taxation on the trade-offs between child labour and school participation for poor households, but through the lens of parents' leisure time. Each parent makes decisions regarding work, consumption, the child schooling time, and his or her own leisure time. Under the income effect, transfers increase a poor parent's income thus consumption and the child schooling time; however, under the substitution effect, labour income tax reduces the opportunity cost of leisure time and transfers make consumption and the child schooling time relatively expensive, hence, lower. If the income effect is stronger than the substitution effect of transfers, the schooling effect is larger when the economy has higher average levels of labour income, or a smaller fraction of poor households (so each will receive a larger amount of transfer), or the income level of a poor family is lower.[7] With that in mind, we examine the moderating effect of average labour income and the poverty headcount ratio on UCT impact in our meta-regressions.

[7] The schooling effect of transfers is also stronger if (i) the parent is more altruistic (i.e., the preference for the child human capital is stronger), or (ii) the parent's preference for his or her own leisure time is weaker, or (iii) the labour productivity parameter for the child relative to his or her parent is smaller (Churchill et al. 2021).

Studies that use randomised controlled trials (RCTs) and other causal inference methods find UCTs improve student participation (see Oosterbeek et al. (2008), Baird et al. (2011), Covarrubias et al. (2012), Benhassine et al. (2015), and Akresh et al. (2016) for RCTs; de Carvalho Filho (2012) and Ponczek (2011) for difference-in-differences; Skoufias and McClafferty (2001), Attanasio et al. (2010) and Bergolo and Galván (2018) for regression discontinuity designs; Veras Soares (2010) and Coetzee (2013) for propensity score matching). When compared to CCTs, the schooling effects of UCTs are smaller but the differences in the effects are statistically insignificant (Baird et al. 2014, Benhassine et al. 2015 and Akresh et al. 2016). However, Baird, McIntosh, and Özler (2011) find that, in Malawi, UCTs can reach out to adolescent girls who fail to satisfy the required conditions of CCTs, drop out of school and are less likely to come back; Akresh et al. (2016), on the other hand, argue that conditionality benefits "marginal children" such as girls, younger children, and lower ability children who are initially less likely to go to school.

One argument for UCTs is that poor households simply lack money, and they can make optimal schooling decision when they become richer (Hanlon et al. 2010). UCTs that strengthen the financial position of poor households would allow them to increase consumption and investment in education. Oosterbeek et al. (2008), for example, find this pure income effect of a cash transfer of $15 per month for households in the first quintile of the poverty index in Ecuador. The transfer increases school enrolment of the group by ten percentage points but has no effect for less poor households. In Malawi, Covarrubias et al. (2012) find a cash transfer increases school attendance and investment in household-oriented productive farm or non-farm activities. Child participation in works outside of the household decreases, but the time freed seems to be replaced with greater within-household tasks (e.g., household chores and productive activities) to help their busy parents. Other forms of cash transfer such as poverty reduction programmes (see, e.g., American Institutes for Research (2014) for a child grant in the Republic of Zambia; Lehmann and Masterson (2014) for a winter cash transfer programme for Syrian refugees in Lebanon; and Sessou and Henning (2019) for a poverty and food insecurity reduction cash transfer in Mali), pension programme (see Martinez (2004) in Bolivia; Ponczek (2011) and de Carvalho Filho (2012) in Brazil) also have the income effect on children's school participation.

The net effect of UCTs on student participation depends on programme features and design. Since demand for schooling involves intertemporal decision-making, it responds differently to a rise in permanent income and to a rise in transitory income. A larger transfer size or a well-established programme could raise permanent income, which should then lead to a greater increase in student participation, while a smaller transfer and those in a pilot stage may not (Baird et al. 2014; García and Saavedra 2016). However, this is contradictory to Oosterbeek et al.'s (2008) finding that a small transfer makes a huge catch up in enrolment levels of children from poor household; thus, they argue that an increment is unlikely to have an impact. On account of heterogeneity of effects across UCTs, we add transfer size and whether the UCT is pilot as moderators to our meta-regression model.

Systematic reviews on cash transfer programmes find positive schooling effects, but only three of the six include a meta-analysis and none has focused solely on UCTs. There are three systematic reviews: Parker et al. (2008) find CCTs, with prime focus on *Progresa*, increase human capital investment (health and education), reduce child work, and improve household consumption; Fiszbein et al. (2009) find 12 of the 13 CCTs increase student participation; and Adato and Bassett (2009) review ten CCTs and ten UCTs and find positive effects on education as well as health, food consumption and nutrition. The three meta-analyses are: Baird et al. (2014) systematically compare the effects of UCTs and CCTs; they find both improve the odds of being enrolled in and attending school compared to no cash transfer programmes. The effect sizes for student participation are always larger for CCTs compared to UCTs, particularly those with more stringent conditions and enforcement, though the difference is not statistically significant. García and Saavedra's (2017) findings on the schooling impact of 47 CCTs corroborate. Snilstveit et al. (2016) get similar results from 38 unique cash transfer programmes (only three are UCTs); however, they argue that having a mother or female head of household as required payee correlates with effect sizes in enrolment, while Baird et al. (2014) and García and Saavedra's (2017) find no correlation between programme features and effect sizes in student participation. This paper adds to the existing literature by conducting the first meta-analysis for 38 studies from 22 UCTs, focusing on school enrolment and attendance.

3. Data

3.1 Data collection

We construct a list of all UCTs around the world using the World Bank's (2015, 2018) Social Safety Net Inventory that contains information on social safety net programmes for 142 countries. We retrieve all programmes under the "Unconditional Cash Transfer" label and compile them and the country name in a spreadsheet. We identify 157 unique programmes in 131 countries. Next, we use Google Scholar and EconLit to search for impact evaluation studies of each programme and we consider those in English only. We use the following key search terms: "*[COUNTRY]* cash transfer evaluation", "*[PROGRAMME]* evaluation", and "*[COUNTRY] [PROGRAMME]* evaluation". We obtain a total of 152 studies for 90 programmes in 64 countries. Additionally, to ensure that we do not leave out any studies, we cross-validate our list with the reference lists of Baird et al. (2014), García and Saavedra (2017), and Bastagli et al. (2019), and add 12 studies to our list – we now have 164 studies. The literature search identifies both established and pilot programmes, which allow our meta-regressions in section 4 to consider the income effect from a rise in permanent income and a rise in transitory income.

We then select eligible studies based on three criteria: First, studies must report common metric characteristics such as regression-based estimates of student participation (enrolment or attendance), and t-statistic, standard errors or p-values that could be converted to effect sizes weighted by their standard errors. Two, we include *ex post* evaluation studies that utilise a treatment-comparison research design (experimental or quasi-experimental) only; we exclude those that use structural models or simulations for *ex ante* evaluation. Third, we include all studies published in academic journals as well as 'grey literature' consisting of unpublished working papers, technical reports, conference papers, and dissertations. We include unpublished studies which, according to Stanley and Doucouliagos (2012), tend to be newer studies that use newer data (and UCTs). We identify 52 studies that report effect sizes on either enrolment or attendance; among them, only 43 report an error statistic and are eligible for inclusion in the meta-analysis, but three of them use *ex ante* simulations and two are duplicates (previous working versions of a published study), leaving a total of 38 eligible studies between the years 2004 and 2022 in our final sample. The spreadsheet containing information on all programmes and studies collected at each stage of the literature search is available online at https://bit.ly/UCTsynthesis2022.

3.2 Coding Effect Sizes and Standard Errors

Following García and Saavedra's (2017) approach, we take the "best" effect size estimate of student participation in each paper from the most complete model – that is, with the most comprehensive set of control variables. All effect sizes are measured in percentage point change in the probability of being enrolled or attending school for a child of UCT recipient household compared to a child in the control group; hence, comparison of the effect size estimates between studies is straight forward by using log-odds ratio. Some studies report programme effects for multiple non-overlapping subgroups: for example, de Carvalho Filho (2012) reports separate effect sizes of the Old Age Pension in Brazil for boys and girls; Econometría Consultores (2020) report separate effect sizes of the Targeted Social Assistance in Georgia for different income groups; and Santana (2008) reports separate effect sizes of the South African Child Support Grant for different age groups. In such cases, we synthesise the "best" effect sizes of the subgroups into an average using a fixed-effect meta-analysis model. For studies that do not report the standard error, we convert t-statistics or p-values into standard errors.

3.3 Coding Moderating Variables

We construct and code a total of six moderators for our meta-regression model. First, we create a binary variable of whether the programme is a pilot or an established programme at the time of evaluation. Second, we record the transfer amount in each paper in 2010 U.S. dollars and divide that by the average monthly labour income in the economy from the International Labour Organisation of the country in that year to facilitate comparison across UCTs. Third, we use the natural log of the average monthly labour income in the economy as a moderating variable. We take logs because changes in income are often multiplicative rather than additive, for example, a change in income from \$100 to \$200 is more akin to a jump in income from \$1000 to \$2000 than from \$1000 to \$1100. Fourth, we obtain the poverty headcount ratio in the year the UCT is implemented from the World Bank. When the data on the poverty headcount ratio is unavailable for a specific year, we record available data of the closest year. The final two moderating variables are the gender of UCT recipient in the household and the schooling level of the children of UCT recipient households. We code a binary variable set to one when the transfer recipient is female and zero when the transfer recipient is male. Analogously, we code a binary variable set to one when the effect size is reported for children of secondary school age, and zero when the effect size is reported for children of primary school age. In the subsample analysis, we drop observations for when the transfer recipient or the schooling level of children is unspecified. The effect sizes for subsamples in each study are recorded and used as the units of analysis. For example, the Kenya CT-OVS Team (2012) reports the effect of UCT on three subsamples, university students, secondary age children, and primary age children. In this case, we code the different effect sizes for each subsample and use them as the level of analysis instead of the main effect reported in the study when running the meta-regression on children schooling level.

3.4 Coding Risk of Bias

Since we meta-analyse experimental and quasi-experimental studies, we assess the risk of bias for the studies following the procedure in Baird et al. (2014), which employs a risk of bias tool developed by the International Development Coordinating Group (IDCG). We use five categories to determine the *overall risk of bias* of each paper: a paper is classified as high risk of bias if it satisfies fewer than three categories; medium risk of bias if three categories; and low risk of bias if more than three categories. The five categories of the issues of concern are as follows; we code the paper as "yes" if it addresses the issue or "no" if otherwise:

(1) *Selection bias and confounding in programme designs:* the study must eliminate any potential bias in the process of allocating units into the treatment group through random assignment; otherwise, the bias must be corrected for with an appropriate quasi-experimental approach. Most studies that use national survey data do not discuss their attempt to address the issue; only 17 studies (44.7 percent) of the analysis sample satisfy this requirement.

(2) *Absence of spillovers, crossovers, and contamination:* studies must address spillovers from the treatment to the control group through geographic or social separation. All but one study satisfies this category.

(3) *Outcome reporting:* a study satisfies this category if results for all relevant outcomes are reported, and there is no apparent selection in reporting outcomes. Nearly all studies (34 studies or 89.5 percent) satisfy this category.

(4) *Analysis reporting:* a study satisfies this category if it uses a credible analysis method and give an exposition of the reason for using the method. 29 studies (76.3 percent) satisfy this category.

(5) *Other risks of bias:* This is the most subjective of the five categories. It includes channels through which there is a possibility that the results reported by the paper are biased, such as retrospective collection of baseline data, use of an inappropriate instrument or a different instrument for the control and treatment groups, collection of information after different follow-up periods for control and treatment groups, and so forth. Ten studies (26.3 percent) satisfy this category.

3.5 Summary Statistics

Table 1 presents the summary statistics of our analysis sample, which consists of 38 studies of 22 UCTs in 18 countries. Specifically, the sample includes 17 journal articles, 11 working papers, four technical reports, one conference paper, two doctorate theses, two master's theses, and one undergraduate dissertation. More than three-quarters of the analysis sample, or 30 studies, report an effect size for enrolment, while 12 studies report an effect size for attendance. More than half of the studies evaluate UCTs in Africa; nine studies in Latin America and the Caribbean; and the remainder are in the Middle East (four), South Asia (two), and Eastern Europe and Central Asia (two).

Table 1 Summary statistics

	Number	%
Publication type		
Journal article	17	44.7
Working paper	11	28.9
Technical report	4	10.5
PhD thesis	2	5.3
Master's thesis	2	5.3
Undergraduate dissertation	1	2.6
Conference paper	1	2.6
Total number of studies	38	
Reports effects on		
Enrolment	30	78.9
Attendance	12	31.6
Regional distribution		
Africa	21	55.2
Latin America and the Caribbeans	9	23.7
Middle East	4	10.5
South Asia	2	5.3
Eastern Europe and Central Asia	2	5.3
Programme characteristics		
Pilot programme	10	26.3
Randomised controlled trial	7	18.4
Female recipient	12	31.6
Male recipient	5	13.2
Primary school children	16	42.1
Secondary school children	12	31.6
Risk of bias		
Selection bias and confounding - Yes	17	44.7
Spillovers, crossovers, and contamination - Yes	37	97.4
Outcome reporting - Yes	34	89.5
Analysis reporting - Yes	29	76.3
Other risk of bias - Yes	10	26.3
Overall risk of bias - Low	14	36.8
Overall risk of bias - Middle	15	39.5
Overall risk of bias - High	9	23.7
	Mean	**SD**
Programme characteristics		
Transfer amount as a % of average labour income	0.044	0.039
Country characteristics		
Average labour income	833.52	560.68
Poverty headcount ratio	41.7	16.3

Note: Transfer amount and average labour income are in 2010 U.S. dollars. The list of the studies in the sample is available at https://bit.ly/UCTsynthesis2022.

In terms of the quality of study, 14 studies (36.8 percent) have a low risk of bias, and 15 studies (39.5 percent) have a medium risk of bias. Nine studies (23.7 percent) have a high risk of bias and are excluded from the meta-analysis in the sensitivity analysis.

4. Methodology

4.1 Meta-Regressions

We use a random-effects model (Eq. 1) to get the mean UCT effect size (ES_{UCT}) on student participation. Random-effects meta-regressions allow for the true effect size of each paper to vary due to heterogeneity in observed variables such as the sample of participants, programme designs, and programme implementation (Borenstein et al. 2009).

$$ES_{UCT} = \frac{\sum_i w_i ES_i}{\sum_i w_i} \tag{1}$$

where ES_i is the effect size of paper i and w_i is the associated weight for the i estimate, which is given by:

$$w_i = \frac{1}{\hat{\sigma}_i^2 + \tau^2} \tag{2}$$

where $\hat{\sigma}_i^2$ is the within-study variance, or the square of the standard error reported in paper i, and τ^2 is the between-studies variance which can only be computed if the true effect sizes of all paper are known. Stata uses the DerSimonian and Laird (1986) method to obtain a sample estimate for τ^2:

$$\hat{\tau}^2 = \frac{Q - (k-1)}{C} \tag{3}$$

where Q is the weighted sum of squares of the effect sizes reported by paper i, $k-1$ is the degrees of freedom or the number of studies minus one, and C is a factor to standardise the estimate into the same index as the within-study variance.

$$Q = \sum_{i=1}^{k} w_i ES_i^2 - \frac{(\sum w_i ES_i)^2}{\sum w_i} \tag{4}$$

$$C = \sum_{i=1}^{k} w_i - \frac{\sum w_i^2}{\sum w_i} \tag{5}$$

All computations are run on Stata utilising the meta-analysis package.

4.2 Assessing Heterogeneity in Effect Size Estimates

To check if the effect size varies between studies, we compute the I^2 statistic to show the extent of heterogeneity. It is given by:

$$I^2 = \frac{\hat{\tau}^2}{\hat{\tau}^2 + \hat{\sigma}^2} \tag{6}$$

where $\hat{\sigma}^2$ is the meta-analysis error variance, which is computed by Stata along with the overall effect size, ES_{UCT}. The I^2 statistic indicates the percentage of all variability in effect size estimates that is due to heterogeneity (Higgins and Thompson, 2002). For interpreting the value of I^2, we follow the widely used benchmarks of 25, 50, and 75 percent, respectively, representing small, moderate, and high levels of heterogeneity (Higgins et al. 2003).

4.3 Analysing Heterogeneity in Effect Sizes

To test how programme features and country-specific characteristics explain heterogeneity in effect size estimates, we estimate the following study-level meta-regression:

$$ES_i = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \beta_3 x_{3i} + \beta_4 D_{pilot} + e_i \quad (7)$$

where ES_i is the effect size of study i, x_{1i} is the average labour income, x_{2i} is the poverty headcount ratio, x_{3i} is the transfer amount as a percentage of average labour income, D_{pilot} is a binary variable equal to one if the programme is a pilot programme, and e_i is the error term. We expect coefficient of β_1 and β_3 to be positive if average labour income and transfer amount increase the effect size of UCTs for study i; we expect poverty headcount ratio and the pilot programme correlate with a smaller effect size for study i (β_2 and β_4 are negative) (García and Saavedra 2017; Churchill et al. 2021).

5. Results

5.1 Meta Analysis

Table 2 reports the overall UCT effect sizes obtained from a random-effects meta-analysis for student participation and heterogeneity statistics: enrolment in row (1), attendance in row (2), and specifications that exclude studies with high risks of bias in rows (3) – (4). We find that the odds of children being enrolled in school and those of attending school are higher for UCT households; the results are robust to the exclusion of studies with a high risk of bias. The estimated overall effect size for enrolment in log odds ratio is 0.042 (the odds ratio is 1.043; 95 percent confidence interval [CI] 0.029-0.055), suggesting that the odds of children being enrolled in school is 4.3 percent higher among children in UCT households than that of those in non-UCT households. UCTs are also associated with an increase in school attendance: the odds that a child attends school is 3.6 percent higher for UCT households as compared to non-UCT households. For a robustness check, we exclude studies with a high risk of bias, and the log odds ratios increase slightly and remain statistically significant at the 1 percent significance level for enrolment and at the 10 percent level for attendance.

Table 2 Meta-analysis results

		Overall Effect Size (p-value)	95% confidence interval	τ^2	I^2	Chi-squared statistic (p-value)	N
Enrolment	(1)	0.042 (0.000)	[0.029, 0.055]	0.001	0.722	103.98 (0.000)	30
Attendance	(2)	0.035 (0.0176)	[0.006, 0.064]	0.001	0.760	45.88 (0.000)	12
Enrolment (Studies with high risk of bias excluded)	(3)	0.060 (0.000)	[0.040, 0.081]	0.001	0.717	77.69 (0.000)	23
Attendance (Studies with high risk of bias excluded)	(4)	0.037 (0.051)	[0.000, 0.075]	0.002	0.701	30.11 (0.000)	10

Notes: Overall effect sizes, p-values, and 95-percent confidence intervals are computed using a random-effects meta-analysis model on Stata. Overall effect sizes are in log odds ratios and their standard errors are in parentheses. In the third and fourth columns, studies with high risk of bias are removed from the meta-analysis. The between-studies variance τ^2 is estimated using the DerSimonian-Laird method. I^2 statistics indicate the percentage of all variability in effect size estimates that is due to heterogeneity. Chi-squared statistics for homogeneity test are presented with corresponding p-values. N denotes the number of studies used in the meta-analysis.

The third to fifth columns of Table 2, which also present heterogeneity statistics, show evidence of the effect sizes are statistically different between studies. The estimates of the between-study variances, τ^2, are 0.001, indicating that between-study heterogeneity exists in the data and that the random-effects model is a more appropriate choice.[8] The I^2 statistics are close to 70 percent, implying that about 70 percent of all variability in the effect size estimates is due to between-study heterogeneity. The Chi-squared statistics for homogeneity test and their corresponding p-values suggest a rejection of the null of homogeneity in effect sizes between the studies.

[8] The value of τ^2 measures the variance of the distribution of true effect sizes, which is included in Eq. (2) for calculating an adjusted random-effects weight for each observation.

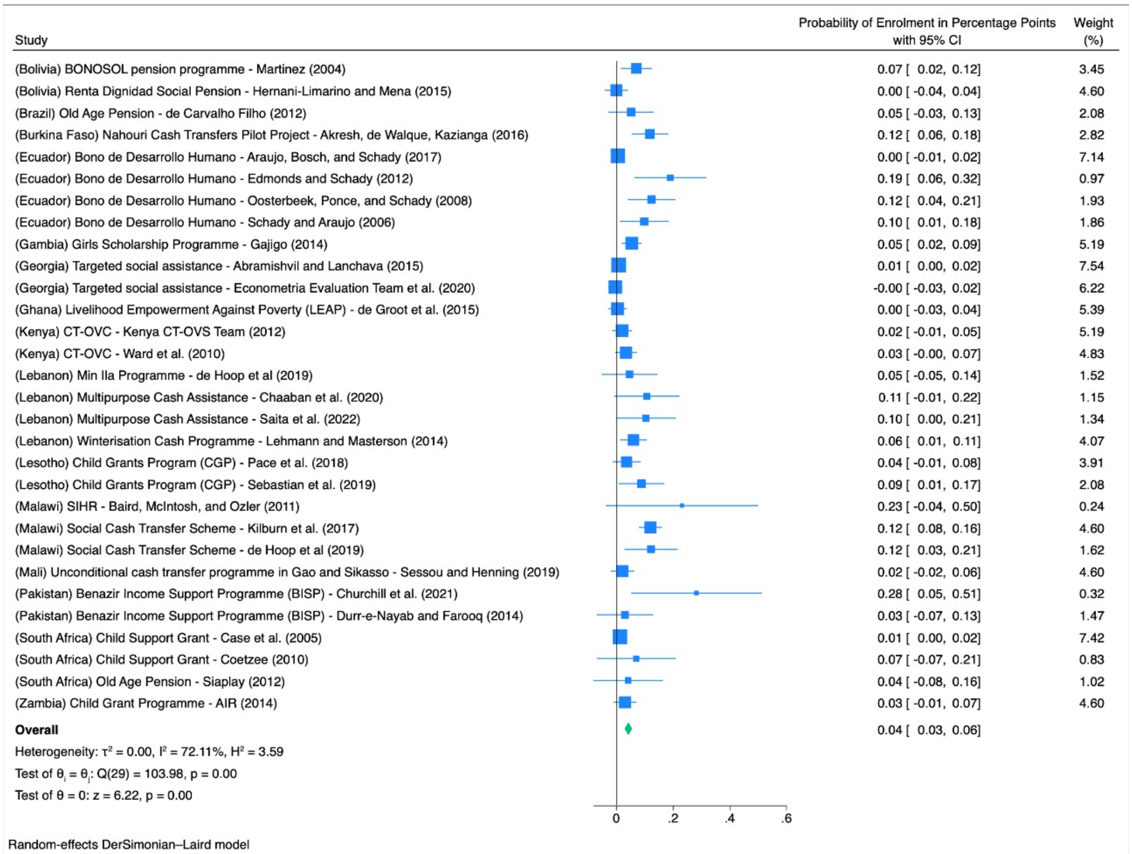

Figure 1 Impact of UCTs on enrolment[9]

Notes: The blue squares denote the individual study effect sizes for enrolment and the blue horizontal lines are the corresponding 95 percent CIs. The size of the squares denotes the weight of the study in the meta-analysis, with larger squares indicating a larger weight (more precise study). The green diamond indicates the overall effect size summarised by the meta-analysis. The vertical line is at zero. The list of the studies in the sample is available at https://bit.ly/UCTsynthesis2022.

Figures 1 illustrates the corresponding forest plot for enrolment, which lists individual effect sizes and the overall effect size (in percentage points instead of log odds ratios), their 95-percent CIs, weights, and heterogeneity statistics. Each study corresponds to a square centred at the point estimate of the effect size with the horizontal line depicts CI; the area of the square is proportional to the corresponding study weight. The overall effect size corresponds to the diamond centred at the estimate of the overall effect size; the width of the diamond corresponds to the width of the overall CI.

[9] Nine studies were included in Baird et al. (2014): Schady and Araujo (2006), Oosterbeek et al. (2008), Coetzee (2010), Ward et al. (2010), Baird et al. (2011), The Kenya CT-OVS Team (2012), de Carvalho Filho (2012), Edmonds and Schady (2012), Akresh et al. (2016).

Most studies report a positive effect on school enrolment though only 11 studies find a statistically significant effect. Exceptions are de Groot et al. (2015), Hernani-Limarino and Mena (2015), Araujo et al. (2017), and Econometría Consultores (2020) that report zero or negative effect sizes, but the estimates are not statistically significant. Figure 1 also reveals that studies with the smallest reported standard errors, and hence with the largest weights in the random-effects model, tend to find effect sizes close to zero. On the other hand, studies that find the largest effect sizes, namely Baird et al. (2011) and Churchill et al. (2021) have the largest standard errors, and hence have less weight in the meta-analysis. The squares for most studies are far away from the diamond, which suggest heterogeneity in the effect sizes between studies, corresponding to the heterogeneity test reported in row 1 of Table 2 and the second line below the diamond of Figure 1.

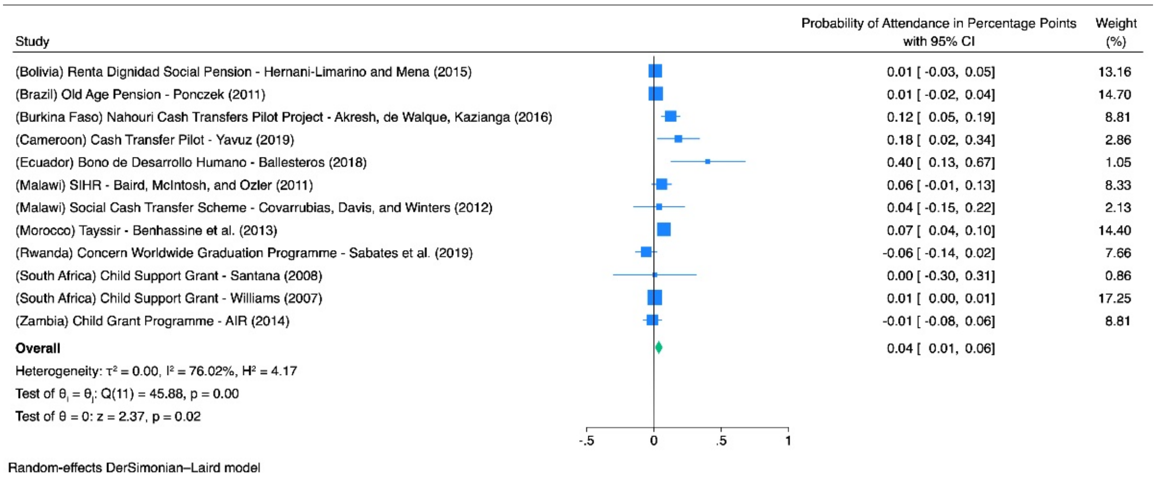

Figure 2 Impact of UCTs on attendance[10]

Notes: The centre of the blue squares denote the individual study-specific effect sizes for enrolment and the blue horizontal lines are the corresponding 95 percent CIs. The size of the squares denotes the weight of the study in the meta-analysis, with larger squares indicating a larger weight (more precise study). The centre of the green diamond indicates the overall effect size summarised by the meta-analysis. The vertical line is at zero. The list of the studies in the sample is available at https://bit.ly/UCTsynthesis2022.

Figure 2 presents the corresponding forest plot for attendance. Despite a smaller I^2, there is considerably greater variance in the attendance effect sizes. For example, Yavuz's

[10] Seven studies were included in Baird et al. (2014): Williams (2007), Santana (2008), Baird et al. (2011), Ponczek (2011), Covarrubias et al. (2012), Benhassine et al. (2013), and Akresh et al. (2016).

(2019) evaluation of the cash transfer pilot in Cameroon reports an 18.1 percentage-point increase in school attendance, and Ballesteros' (2018) evaluation of the *Bono de Desarrollo Humano* in Ecuador reports an even larger 40.0 percentage-point effect. On the other hand, Sabates et al. (2019) and Santana (2008) report negative effect sizes for the Rwandan Graduation Programme and South African Child Support Grant respectively. The negative effect sizes, however, are not statistically significant. Studies reporting the largest effect sizes again have the largest standard errors, meaning that their weight in the random-effects meta-analysis is small.

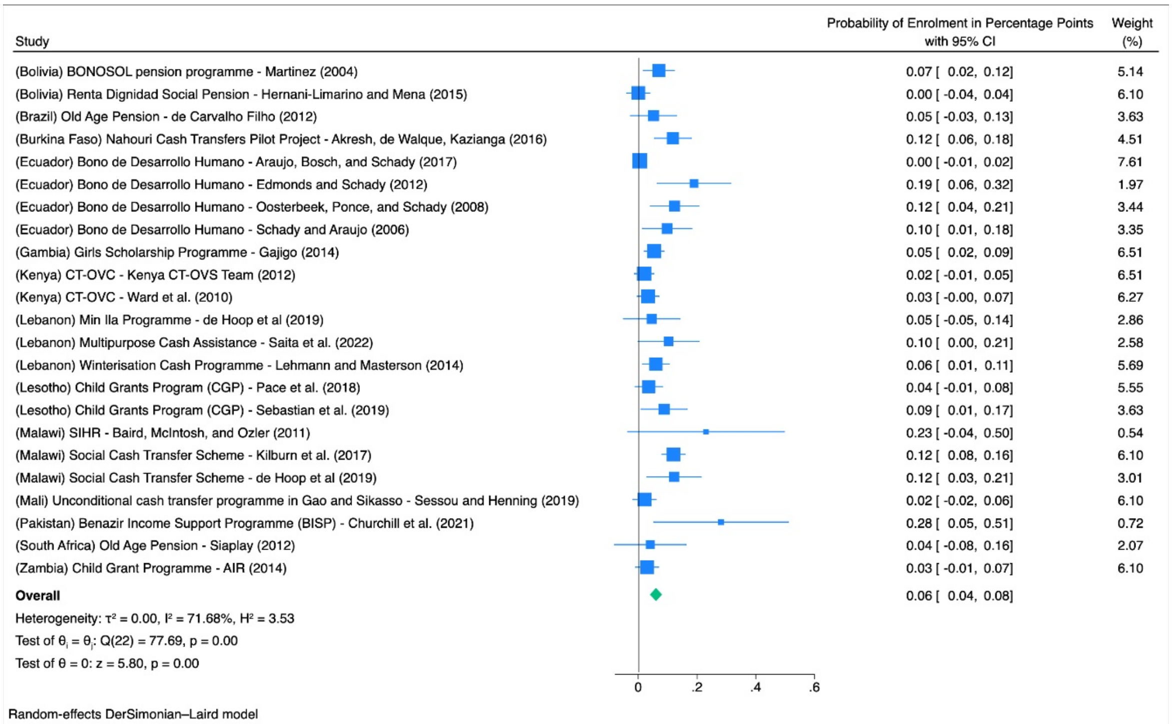

Figure 3 Impact of UCTs on enrolment (high risk of bias studies are excluded)

Notes: The centre of the blue squares denote the individual study-specific effect sizes for enrolment and the blue horizontal lines are the corresponding 95 percent CIs. The size of the squares denotes the weight of the study in the meta-analysis, with larger squares indicating a larger weight (more precise study). The centre of the green diamond indicates the overall effect size summarised by the meta-analysis. The vertical line is at zero.

Figures 3 and 4 show that the basic results are robust to exclusion of studies with a high risk of bias. The overall effect sizes for enrolment and attendance remain positive and statistically significant. We reject the null hypothesis of homogeneity in paper-specific effect sizes for both enrolment and attendance; most of the variation in the effect size is explained by between-study heterogeneity.

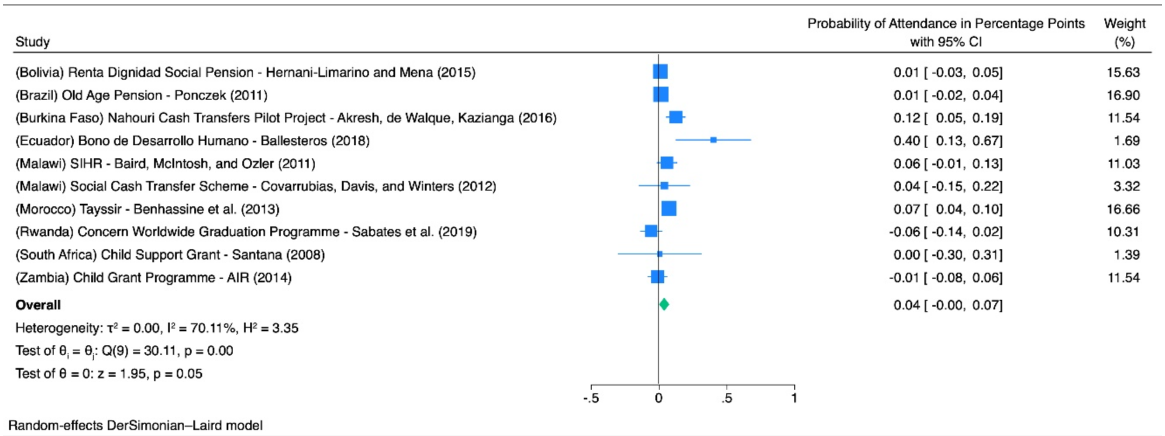

Figure 4 Impact of UCTs on attendance (high risk of bias studies are excluded)

Notes: The centre of the blue squares denote the individual study-specific effect sizes for enrolment and the blue horizontal lines are the corresponding 95 percent CIs. The size of the squares denotes the weight of the study in the meta-analysis, with larger squares indicating a larger weight (more precise study). The centre of the green diamond indicates the overall effect size summarised by the meta-analysis. The vertical line is at zero.

In summary, our results indicate that UCTs increase student enrolment and attendance. However, the overall effect sizes seem to be smaller compared to those in Baird et al. (2014). Some large and positive individual effect sizes on enrolment in past studies could be the result of UCT recipients erroneously believe the transfers are conditional on schooling (Schady and Araujo 2008; Ward et al. 2010; Edmonds and Schady 2012), which is the case for Ecuador's *Bono de Desarrollo Humano* and Kenya's CT-OVC. For enrolment, most new studies that we add to Baird et al.'s (2014) sample have a larger weight (smaller standard errors) and a smaller effect size. The number of studies on attendance is small and we add five new studies only to the sample. These studies have either a large standard error or a small effect size, reducing the overall UCT effect size in our sample.

5.2 Explaining Between-Study Heterogeneity

In this section, we evaluate the role of six moderators on the estimated effect of UCTs for student participation, namely whether the UCT is a pilot programme, the transfer amount as a percentage of average labour income, average labour income (log-transformed), poverty headcount ratio, whether the transfer was given to a male or female member of the household, and the schooling level of children in UCT recipient households. Table 3 presents the results of a multivariate meta-regression of the main effect sizes of each study on the first four moderators. To assess the moderating effect of transfer recipient gender and schooling level, we use the effect sizes on subsamples as reported by the individual studies. Table 4 presents the results of univariate meta-regressions for the final two moderators.

Table 3 Moderating effect of programme features and country characteristics

	(1)	(2)	(3)	(4)
	Enrolment	Attendance	Enrolment (high risk of bias studies excluded)	Attendance (high risk of bias studies excluded)
Pilot dummy	0.009	0.075	-0.020	0.022
	(0.015)	(0.061)	(0.023)	(0.091)
Transfer amount as a % of average labour income	0.180	-0.476	-0.438	-0.845
	(0.271)	(0.519)	(0.442)	(0.761)
Log of average labour income	-0.021 **	-0.001	-0.034	-0.018
	(0.011)	(0.027)	(0.018)	(0.042)
Poverty headcount ratio	0.048	-0.256	-0.087	-0.597
	(0.043)	(0.208)	(0.089)	(0.396)
Constant	0.148 *	0.171	0.341	0.458
	(0.082)	(0.230)	(0.153)	(0.405)
N	30	11	23	9

Notes: We estimate the effect of moderators on the main effect size of each study (measured in log odds ratios) using a random-effects meta-regression, where observations are weighted by the inverse of the effect size variance and between-study variance. Standard errors are reported in parentheses. *N* denotes the number of studies used in the meta-regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. One less observation for attendance because Morocco has no data for average labour income.

The first row of Table 3 indicates a positive correlation between pilot programmes and UCT effect sizes, implying that UCT effect sizes are larger when the programme is a pilot compared to when they are established programmes. This result runs contrary to theoretical predictions, and it is not statistically significant. The signs of the coefficients for transfer amount and poverty headcount ratio are different when using different measures of student participation. These two moderators are associated with a greater effect on enrolment but a smaller effect on attendance. These associations are again not statistically significant. The estimates in the third row are negative, showing a negative correlation between average labour income and UCT effect size. This suggests that UCTs are more effective in settings where the average labour income is lower, but the estimate is statistically significant for enrolment only.

Table 4 Moderating effects of transfer recipient gender and children schooling level

	(1) Enrolment	(2) Attendance	(3) Enrolment	(4) Attendance
Transfer recipient gender	0.048 (0.045)	-0.023 (0.052)		
Children schooling level			0.037*** (0.014)	-0.008 (0.016)
Constant	-0.006 (0.080)	0.086 (0.090)	-0.014 (0.020)	0.021 (0.021)
N	18	8	32	14

Notes: We estimate the moderating effect of transfer recipient gender (dummy variable equals one if the UCT is given to a female recipient in the household and zero if otherwise) and children schooling level (dummy variable equals one if the UCT is targeted at secondary age children and zero if primary school age children; other schooling levels are excluded in the subsample) on the effect size of each study. We use subgroup effect sizes reported by the papers where possible. Papers where the transfer recipient gender and/or children grade is unspecified are excluded from the meta-regressions. Standard errors are reported in parentheses. *N* denotes the number of reported effect sizes used in the meta-regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

According to Table 4, there is no statistically significant relationship between transfer recipient gender and the effect of UCT on enrolment. Children schooling level, however, is positively associated with UCT effect size, suggesting that UCTs that target secondary age children are more effective. This could be explained by the fact that most countries already have mandatory primary schooling (see https://bit.ly/UCTsynthesis2022). The sign of the coefficient changes when looking at attendance as the outcome, but the estimate is not statistically significant. The bubble plots in the appendix (Figure 5) illustrate the corresponding relationship between the effect sizes and the moderators.

The finding that children schooling level matters, but other programme features explain little of the between-study heterogeneity is in line with Baird et al. (2014). They find, in subgroup analysis and for CCTs only, cash transfers are only effective in increasing the enrolment at secondary level but not at the primary level.[11] They also find no evidence of the effect size of UCTs differs by whether the programme is pilot, the transfer amount, and UCT recipient gender, which is also the case for CCTs (see also García and Saavedra (2017)). For country characteristics, we find average labour income reduces the effect size of UCTs on school enrolment. Based on the theoretical predictions in Churchill et al. (2021), this result may imply that the income effect of UCTs is weaker than the substitution effect. However, we find no evidence that the effectiveness of UCTs on school participation depends on poverty head count ratio. We believe that two reasons explain the findings. Firstly, other unobserved factors or omitted factors may account for a large fraction of the variation in effect sizes across the studies. Secondly, the number of studies included in the meta-regression may be too small for any systematic relationship between the effect sizes and the moderators to emerge.

5.3 Publication Bias[12]

With our best efforts, all studies – both published articles and grey literature – that meet the selection criteria in Section 3.1 are included in the meta-analysis. The results of the meta-analysis are valid insofar, but it is still possible that some relevant studies and nonsignificant

[11] For subgroup analysis by schooling level, Baird et al. (2014) estimate the overall effect size of CCTs and for enrolment only (insufficient observations for UCTs and for attendance). They look at the overall effect size at each schooling level while we use schooling level as a moderator.

[12] We use R for this section as the package has more methods that account for publication bias in meta-analysis.

results are being underreported in the literature. This non-random missing data could be a source of publication bias.

To examine the possibility of small-study effects, Figures 6 and 7 present the funnel plots of UCT effect sizes for enrolment and attendance, respectively, against standard errors. In the absence of small-study effects, we expect to observe a roughly symmetric inverted funnel (Peters et al., 2008) – that is, on the dotted funnel, papers with smaller standard errors are expected to cluster around the top of the funnel and those with larger standard errors to cluster around the bottom. The vertical dotted line in the figures is the reference line and the overall UCT effect size (the overall log odds ratio). The contour-enhanced funnel plots are used to identify if the funnel-asymmetry is because of publication bias or other reasons (e.g., between-study heterogeneity). The grey plotted contour regions define the regions of statistical significance ($p < 0.1, p < 0.05$ and $p < 0.01$) while the white region defines the region of statistical insignificance. If there are studies, especially smaller ones, missing in the nonsignificant regions, publication bias is suspected.

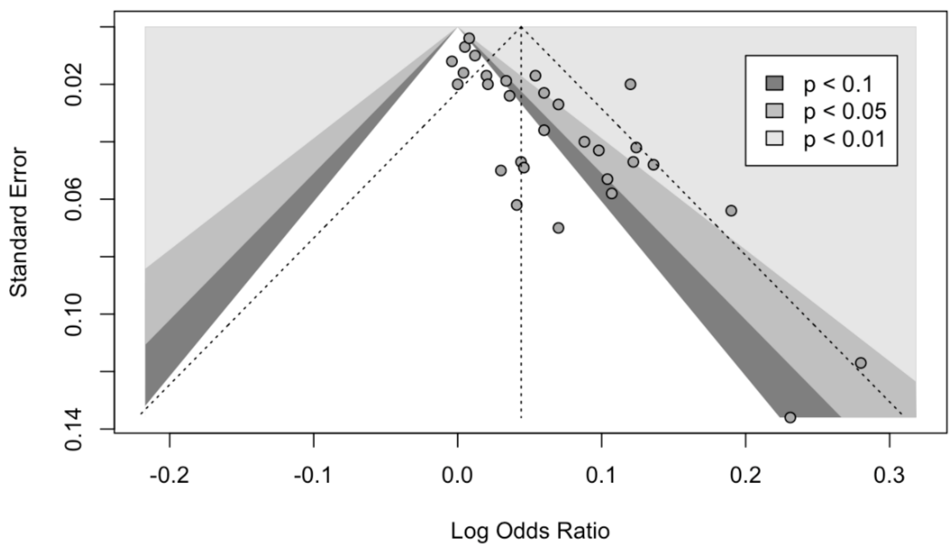

Figure 6 Contour-enhanced funnel plot for meta-regression on enrolment

Notes: The scatterplots are the individual studies effect size. The vertical dotted line in the figures is the reference (overall effect size) line. The grey plotted contour regions define the regions of statistical significance ($p < 0.1$, $p < 0.05$ and $p < 0.01$) while the white region defines the region of statistical insignificance.

Figure 6, which presents the funnel plot of UCT effect size for enrolment, displays a tight cluster of papers near the top of the dotted funnel, but there are also outliers and dots at the right corner that do not have a corresponding paper on the left side of the funnel. The bottom of the non-significant region of the contour funnel is empty, which implies the presence of potential publication bias perhaps because of omission of smaller papers and those with larger standard errors in the meta-analysis. There is also a chance that between-study heterogeneity identified in Table 2 and Figure 1 induce the asymmetry in the funnel plot. An Egger's regression test (Egger et al. 1997) indicates that the asymmetry for enrolment is statistically significant ($p = 0.000$).

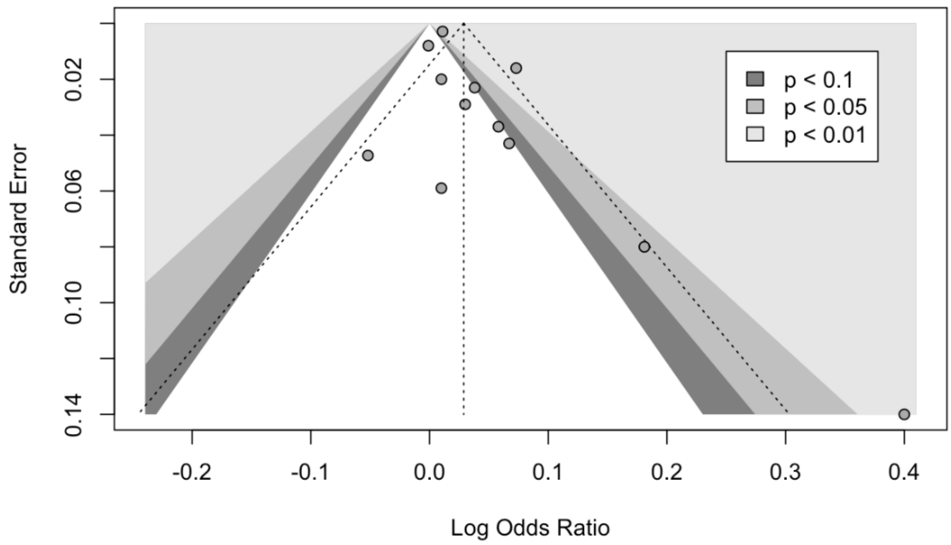

Figure 7 Contour-enhanced funnel plot for meta-regression on attendance

Notes: The scatterplots are the individual studies effect size. The vertical dotted line in the figures is the reference (overall effect size) line. The grey plotted contour regions define the regions of statistical significance ($p < 0.1$, $p < 0.05$ and $p < 0.01$) while the white region defines the region of statistical insignificance.

Figure 7, which presents the funnel plot of UCT effect size for attendance, also show a symmetric distribution of papers at the top of the dotted funnel, but most of the papers report non-significant results (in the white region with p-values larger than 10 percent). This suggests no small-study effect (and publication bias) is suspected. We also fail to reject the null that the funnel plot for attendance is symmetric in the Egger's regression test ($p = 0.0687$).

Table 5 reports the overall UCT effect size for enrolment using three bias-correction methods that account for potential publication bias identified in Figure 5. First, row 1 shows that, after omitting the outliers from the meta-analysis, the overall effect size remains statistically significant. Second, we compute the overall effect-size estimate using the observed and imputed studies through the "trim-and-fill" method (Duval and Tweedie 2000). The estimate, which is statistically significant at the 5 percent level, suggests that the odds that a child being enrolled in school is four percent higher for UCT households. Lastly, we employ Rücker's limit meta-analysis method (Rücker et al. 2011), which adjusts for small-study bias, and get a log odds ratio of 0.016 that is statistically significant at the 10 percent level. We do not assess the impact of publication bias on the results for attendance because of small number of studies.

Table 5 Bias-corrected overall UCT effect size for enrolment

Correction method		Overall effect size (p-value)	95% confidence interval	τ^2	I^2	Chi-squared statistic (p-value)	N
Remove outliers	(1)	0.040 (0.000)	[0.027, 0.053]	0.0005	0.720	96.33 (0.000)	28
Trim and fill	(2)	0.016 (0.030)	[0.001, 0.030]	0.0011	0.787	202.31 (0.000)	44
Limit meta analysis	(3)	0.016 (0.060)	[-0.001, 0.033]	0.0006	0.721	103.98 (0.000)	28

Notes: Overall effect sizes are in log odds ratio. The between-studies variance τ^2 is estimated using the DerSimonian-Laird method. I^2 statistics indicate the percentage of all variability in effect size estimates that is due to heterogeneity. Chi-squared statistics for test of homogeneity are presented with corresponding p-values. N denotes the number of papers used in the meta-analysis.

6. Conclusion

Unconditional cash transfers seem to encourage student participation: on average, the odds of a child being enrolled in school and attending school, respectively, is 4.3 and 3.6 percent higher for households that receive a cash transfer, and the estimates are statistically significant. Analysis that excludes papers with high risk of bias gives similar result: the overall UCT effect sizes for both enrolment and attendance remain positive and statistically significant. We also find evidence of average labour income and schooling level moderates the effect of UCT on enrolment but not on attendance. Other moderating variables – whether the UCT is pilot, transfer amount as a percentage of average household income, poverty headcount ratio, and the gender of recipients – do not seem to explain the between-study heterogeneity identified in the meta-analysis.

Our main findings of UCTs are effective in improving school participation corroborate Baird et al. (2014) though the overall effect sizes we get are relatively small. One reason is that the new studies we add use more rigorous evaluations, and some find a much modest UCT effect size. In addition, a large schooling effect of UCTs could be potentially driven by mistaken perceptions about schooling requirements among UCT recipients (Schady and Araujo 2008; Ward et al. 2010; Edmonds and Schady 2012). Many children or schooling UCTs emphasise the importance of the transfer for human capital accumulation, which creates misunderstanding of the programmes (e.g., Ecuador's *Bono de Desarrollo Humano* and Kenya's CT-OVC). Our findings highlight the need for future UCT evaluations and meta-analyses to take into account perceptions of rules and conditions in estimating the schooling effect of UCTs. This issue is less likely to occur in UCTs that do not directly involve children (e.g., pension programmes), it would be worthwhile to have more assessments on the schooling effect of these UCTs.

In line with Baird et al. (2014) and García and Saavedra (2017), we find no statistically significant moderating effect of programme features; the exception is children's schooling level matters for the effect of UCT on enrolment. J-PAL's (2017) review suggests that if the objective is simply increasing enrolment and attendance at school, smaller incentives – both cash and non-cash transfers – can be just as effective. One possible reason is that student participation is sensitive to the perceived costs and benefits of education. As a result, a small decrease in the perceived costs, including non-monetary costs like travel time, can boost participation. If this is the case, it also helps justify limited empirical support for the moderating effect of whether the UCT is pilot, transfer recipient gender, and other design elements (e.g., baseline enrolment rate, and the frequency of transfer that Baird et al. (2014) and García and Saavedra (2017) find statistically insignificant). Our finding implies that policymakers could use the smallest amount of transfer to attain the average schooling impacts through a "nudge" (Benhassine et al. 2015) or addressing perception gaps in returns to education (Glewwe and Muralidharan 2015; J-PAL 2017).

We find average labour income reduces the effect of UCTs on enrolment but not on attendance. We also find no evidence that poverty headcount ratio moderates the schooling effect of UCTs. Churchill et al.'s (2021) theoretical framework predicts that the moderating effect of the two variables is larger if the income effect is stronger than the substitution effect of transfers. However, the argument is built on the assumption of UCTs are financed by labour income tax, which determines parents' leisure time hence schooling decisions. While we consider the income effect of UCTs, it would have been useful to also account for the substitution effect of UCTs, but it would require details on labour income taxation or parents' income per unit of labour time or their leisure time. This is one of the limitations of this review, which future work could explore.

Our review has two other limitations. First, we include only 22 UCTs in 18 countries although it is a significant increase from five UCTs included by Baird et al. (2014). The World Bank (2015) reports that UCTs are present in 113 countries worldwide, but we only find studies of 90 UCTs in 64 countries and only 38 of them report schooling effects; among them, only 12 assess the effects on school attendance. Our small sample implies that many UCTs in developing countries have not been evaluated, which is a low-hanging fruit for researchers to increase the evidence base for UCTs. Second, while we identify the presence of between-study heterogeneity, it remains unexplained by most of the moderators that we consider. This suggests that unobserved design elements and other country characteristics associated to effect sizes may cause between-study heterogeneity. A larger sample of rigorous UCT studies is still needed to assess short- and long-term schooling effects of UCTs (enrolment, attendance, test scores, future employment of children as adults, and their earnings), their cost effectiveness, and their moderators. A best practice to future studies is to report a corresponding error statistic, study-level characteristics such as programme design features, baseline outcome measurements, respondents' understanding of the programme, as well as other variables in the experimental setting.

Declarations

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CRediT authorship contribution statement

Zhi Zheng CHONG: Conceptualisation, Data Curation, Formal Analysis, Methodology, Software, Validation, Writing-Original Draft, Writing-Review & Editing. **Siew Yee Lau:** Conceptualisation, Supervision, Writing-Review & Editing.

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Data availability statement

The data that support the findings of this study are available at https://bit.ly/UCTsynthesis2022.

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Appendix

Bubble plots

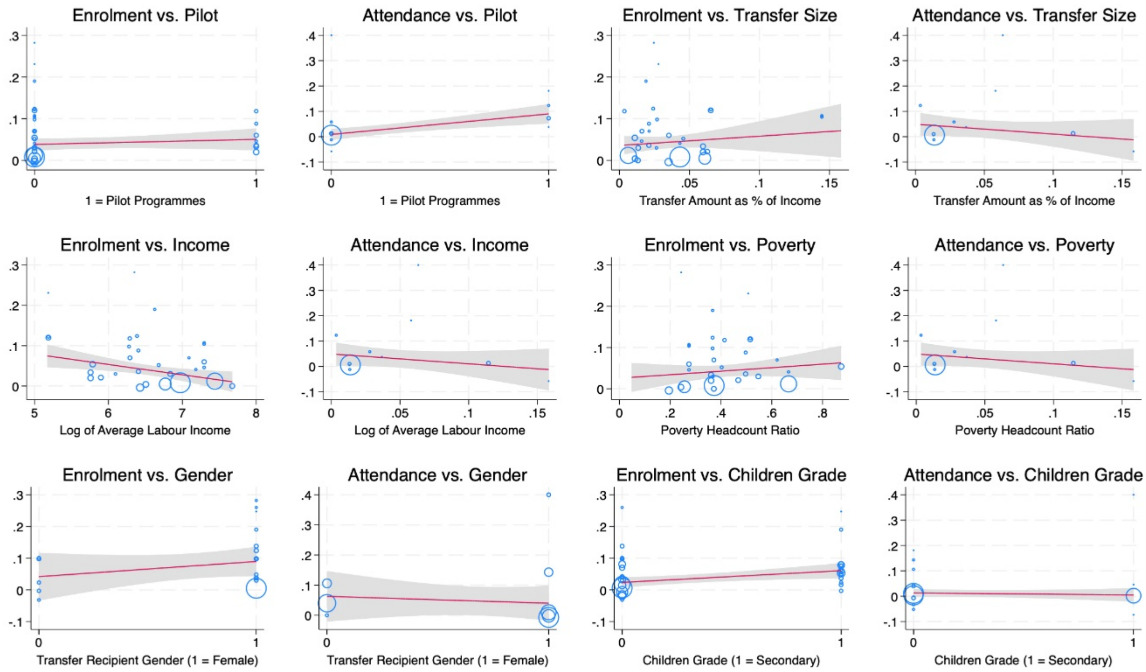

Figure 5 Bubble plots

Notes: Effect sizes for enrolment and attendance in log odds ratios are graphed on the y-axes, while the moderating variables are graphed on the x-axes. Bubbles indicate the observations (effect sizes), and the size of the bubbles corresponds to the weight of the observation (inversely related to its standard error). The red lines denote a linear predicted regression, and the grey shaded areas denote the corresponding 95-percent confidence interval.